Robert Taylor Interview
28 February 1989

Abstract

Following a brief description of his academic and professional background before joining ARPA, Taylor describes his impression of the IPT Office when he arrived in 1965.

Most of the interview is concerned with the IPT Office under Taylor's tenure as Director from 1966 to 1969. The major issues he discusses are: his understanding of the overall mission of the IPT Office at that time; the working relationship between IPTO and the Director of ARPA; the effect of external events, such as the Viet Nam war, on IPTO; how the budget process operated; what general areas of research were funded by IPTO; how IPTO compared to other government agencies in its funding of research in computer science; what sort of relationship the IPT Office maintained with its various contractors; and finally, how Taylor's successor was recruited to head the IPT Office. The interview ends with Taylor's general observations on how IPTO and ARPA have changed since their early years.

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ROBERT TAYLOR INTERVIEW

DATE: 28 February 1989 INTERVIEWER: William Aspray

LOCATION: Palo Alto, CA

ASPRAY: This is the 28th of February, 1989 in Palo Alto, California. Let me begin the interview by asking you if you can tell me briefly about your own background in computing and how you got involved with the people at ARPA.

TAYLOR: You said "briefly"; it might be hard to make it extremely brief, because there are a couple of circumstances that caused this to happen. First of all, when I was in graduate school there was no computer science, so I didn't really have much of an introduction to computing. I had to use it occasionally but not heavily. I did my graduate work at the master's level in psychoacoustics.

ASPRAY: This was in Austin?

TAYLOR: Yes, at the University of Texas. We were trying to build a model of the auditory nervous system as it functioned in the cases of sound localization and masking of signal against noise background, and doing some research in applied undersea warfare under sponsorship of the Navy as well. So, we were doing both basic and applied research in acoustics. Now, one of the most famous names in acoustics at that time was J.C.R. Licklider, so of course I knew of him as a graduate student, knew of his work. Then some years later I found myself, after being a systems engineer in the aerospace industry for a while, head of a new NASA program in the NASA headquarters Office of Advanced Research and Technology, and this program included funding research in manned-flight control systems, displays, and simulation technology in the NASA centers and in universities elsewhere. I was there from 1962 to early 1965. While there I was supporting some computer research. Doug Englebart's work at SRI, for example, was one of the projects that was funded out of my office. In late 1962 Licklider came to ARPA. Shortly after he got there he formed an unofficial committee of people

in the government who were funding computer research, so that they could exchange information about who they were funding, and look for opportunities to collaborate, and remove undesirable overlap. That sort of thing. So, I was one of the members of Licklider's committee.

ASPRAY: Were you chosen because of...?

TAYLOR: Because of my NASA work. I was funding some computer work and Licklider knew about it.

ASPRAY: In particular, it wasn't because of your mutual research interests in psychoacoustics?

TAYLOR: No, but when the message came from Licklider, it was because of my acoustics research background that I realized who he was. Also, while he was at MIT he [advised] a thesis in manned-flight control systems related work, which was one of the other domains I was funding research in. So I knew of Licklider's coming to ARPA through that connection as well, but I had never met him personally. My thesis advisor at the University of Texas was a good friend and admirer of his. So these were indirect connections. So Licklider formed this government committee, a committee of people who were in the government and who were funding computer research, and we had several meetings. Through those meetings and after an occasional meeting when I could visit with him personally, I got to know him better. He had recruited Ivan Sutherland to come and be his successor at ARPA...

ASPRAY: Middle of 1964?

TAYLOR: I think that's right. Then early in 1965 Ivan, I think with Licklider's recommendation, asked me to leave NASA and come over to ARPA and be Ivan's deputy director. Then in 1966 Ivan left ARPA, and I was moved to the position of Director of Information Processing Techniques.

ASPRAY: I see. So when I've seen in some of your biographical information that you were Research Director of Computer Science, that's equivalent to what you just told me as Deputy Director of IPTO.

TAYLOR: Yes. I was Deputy Director for a year, or maybe a little less than a year. Then I was Director for the rest of the time until I left in late 1969.

ASPRAY: Why don't you tell me what the program at IPTO looked like when you arrived. What sorts of projects were under way? What was your reaction to them? Did you seem to think that they were doing things correctly?

TAYLOR: First of all, the reason I moved from the NASA position is fundamentally because over time, I became heartily subscribed to the Licklider vision of interactive computing. The 1960 man-computer symbiosis paper [that Licklider wrote] had had a large impact on me. Have you read that?

ASPRAY: Yes, I have seen that. Are you uncommon in having had that paper influence you?

TAYLOR: I don't think so. I don't really know how many other people it influenced, but it certainly influenced me. The Vannevar Bush work published in *Atlantic Monthly* 1945 had an influence: "As we may think." I probably didn't run into that paper until long after it was published. I probably ran into it about the same time I ran into Licklider's paper. Some of the work of Norbert Wiener in the late 1940s also had some influence. I was interested in the junction of these three different themes. My graduate work had been concerned with how the nervous system works. Over time I began to feel that the kind of computing research that Licklider was suggesting was going to be much more rewarding in terms of the relative immediacy of feedback as compared with the slow, arduous process of trying to understand the central nervous system and the brain. Some of us need more instant feedback and reinforcement than others. So I gradually got more and more interested in computer research, and less and less interested in brain research.

ASPRAY: Yes.

TAYLOR: How did the program strike me? Well, first of all you must understand that when Licklider brought together this government committee of all the people who were supporting computer research in one form or another, and told us about this very modest program that he had just gotten underway, his "modest" budget was greater than the sum of all the other budgets represented on that committee. It was $15 million a year, which by today's standards is certainly modest. It had in it Project MAC at MIT, Project Genie at Berkeley, a time-share project at the Systems Development Corporation of Santa Monica, and other projects that I can't remember. But I think those -- the MIT and SDC projects -- were the largest monetarily. The Berkeley project in the early days was pretty small; it got larger later.

ASPRAY: Before we move on, can you give me an idea of some of the other organizations represented, or some of the other people that were on this government committee?

TAYLOR: Yes. ONR was represented; I can't remember now the name of their representative. The Air Force Office of Scientific Research was represented by Harold Wooster and Rowena Swanson. There was an Army Research Office out at Durham, North Carolina that was represented. I represented NASA. Army, Navy, Air Force, The National Institute of Health were represented by... I can't remember his name either. I haven't thought about these things in a long time. What other groups? I think that was it. Would there be any other groups that would be logically considered then? We have the Army, the Navy, the Air Force, the National Institutes of Health...

ASPRAY: Possibly the Bureau of Standards?

TAYLOR: No, I don't thing Bureau of Standards at that time was supporting computer research; I think that happened later. I think that was it.

ASPRAY: All right.

TAYLOR: I thought that the early ARPA program, as put in place by Licklider, was very well conceived. As I got to meet the people that he was supporting, I even thought more highly of the program, because these were very good people. They were smart; they were dedicated; they were committed. Indeed, I essentially continued funding the groups that he started, and in fact enlarged most of it.

ASPRAY: When did you come into contact with Sutherland?

TAYLOR: After he was at ARPA he invited me to come over and talk to him about the program. He'd known that I had been part of this government committee. He'd known that I had been funding Englebart. After Licklider got his program under way, he also funded Englebart. I suppose Ivan contacted me because he knew of the joint interests manifested through the committee work, and he was looking for a deputy.

ASPRAY: As I understand it, Licklider had an administrative aid, but not a scientific one -- a military man whose name I can't recall.

TAYLOR: That's right. I think by the time Ivan got to ARPA that chap was gone.

ASPRAY: What was the perceived need for a scientifically oriented deputy in the office? Had the program grown enough by that time that there was a feeling that this was needed, or were there special projects in mind?

TAYLOR: No, we didn't divide up the work in any sense at all. When I joined Ivan he was by himself in the office, except for a secretary. So, even if the program had been restricted to the same size as it was when Licklider left, Ivan would have needed some help. I have no idea what the deliberations over scientific versus administrative help were, if [there were] any.

ASPRAY: Yes. So, as deputy director, what were your major duties? Were you on the road more than Ivan?

TAYLOR: No, I was with Ivan most of the time.

ASPRAY: Oh, you were with Ivan.

TAYLOR: We worked together on all aspects of the program; we didn't divide any of the duties.

ASPRAY: Okay. When you compare Sutherland's tenure as the director to Licklider's do you see a different philosophy or management style?

TAYLOR: I wasn't at ARPA when Licklider was at ARPA, so I can't make that comparison. I very much enjoyed working with Ivan. He was very smart; he paid a lot of attention to detail; he was honest, forthright.

ASPRAY: Had he had new initiatives other than the programs that had already been established by Licklider?

TAYLOR: Yes, he did; I won't be able to remember all of them. The largest one in terms of dollars was probably ILLIAC 4. I think Ivan believed that it was important to give some support to new, ambitious machine architecture projects. Licklider had not been funding those kinds of projects. So, the ILLIAC 4 project was certainly something that happened on Ivan's watch. The graphics work at University of Michigan was under Sutherland's initiative as well. The CMU sponsorship probably started under Lick. I had forgotten about that when I was mentioning the early projects. I don't know how early that one was, but I believe Lick started that. If not, Ivan started it. I believe Ivan may have picked up some graphics work again at Rand that resulted in the Grail project, and the Rand Tablet. Lick certainly was interested in that. Ivan might have inherited it; I can't remember. After I got there he wasn't there very long. Within a year or so he had gone to Harvard.

ASPRAY: As I understand, he left in June of 1966.

TAYLOR: I think that's right, yes.

ASPRAY: Were you the obvious choice to take over?

TAYLOR: I don't know. I certainly knew a lot less about computing than Ivan did, or than Lick did. But I had a lot of context about the nature of the program and the people that we were supporting. I presume that Ivan recommended me or I probably wouldn't have been selected.

ASPRAY: Was there any objection on, say, the part of more senior ARPA officials?

TAYLOR: Not that I know of. There might have been, but no one ever told me.

ASPRAY: Well, how would you compare your...? This is a difficult question --I understand that. Your knowledge of computing may have been as great as Licklider's when he arrived at the office.

TAYLOR: No, I don't think so. First of all, Licklider had been playing around with computers some when he was at Lincoln Lab. And then later, when he went to BBN, he built a small group with a number of very good people -- Ed Fredkin, among others. They had built one of the first timesharing systems out at BBN.

ASPRAY: Let me go back and say that the reason I made this suggestion is that when we interviewed Licklider he was very modest about these things, "Oh, all these young men working with me knew all about this, but I was a few steps away from it."

TAYLOR: Well, it's all relative. Licklider is modest to a fault. What he said is true, but he still was a knowledgeable person about scientific endeavor in general, philosophy of science, and he was a very good judge of people. I don't think that Ivan, nor I, nor anyone who's been in that DARPA position since has had the vision that Licklider had. His being at that place at that time is a testament to the tenuousness of it all. It was really a fortunate circumstance. I think most of the significant advances in computer technology, especially in the systems part of computer science over the years -- including the work that my group did at Xerox PARC where we built the first distributed personal computer system -- were simply extrapolations of Licklider's vision. They were not really new visions of their own. So he's really the father of it all. And you'll never get him to admit that, because of his modesty.

ASPRAY: Okay. Let's turn to your tenure as the IPTO director. Can you tell me what you understood to be your mission, especially as it related to the overall military objectives of the agency?

TAYLOR: We were certainly a Defense Department agency, but I never received any guidelines that the research that I sponsored should have a specific military connection. What we were trying to do, on the one hand, was to select research problems that were ripe enough to have some hope of making progress on them, but on the other hand, to select them with the notion that if we succeeded it would make an order of magnitude difference between the way in which business was done then versus what this new research finding might permit. And in a few instances, indeed, that was the case. We can certainly argue that timesharing versus batch processing made probably more than an order of magnitude difference across a huge number of parameters with regard to the way computer technology can serve man. I think the ARPANET is another example of that. But we were explicitly looking for things which, if they could succeed, would really be a large step beyond what technology could then permit. We were not constrained to fund something only because of its military relevance. Without being told by anyone, we would make connections between the research problems that we were attacking and the technology limitations that were faced by the Defense Department. But those problems that were faced by the Defense Department were also faced by a wide variety of other endeavors of the United States and, in fact, the world.

When I convinced Charlie Herzfeld, who was head of ARPA at the time, that I wanted to start the ARPANET, and he had to take money away from some other part of ARPA to get this thing off the ground, he didn't specifically ask me for defense rationale. I gave him a number or reasons why I thought the ARPANET project ought to be launched. Some of them, such as the ability of a computer network to fail softly, were certainly in the interest of the Defense Department, but they also would be in the interest of anyone else who is running a computer network. I'll take it a step further and say that research that's funded primarily on the basis of its mission orientation, whether it's a defense mission, or a health mission, or a space mission, is likely to be weaker as a research project than if it's funded along the lines of what a difference it can make to mankind generally, if it succeeds. I think you've seen a lot of evidence of that. You've seen evidence of many of the research projects that have been funded under direct mission specificity have been weak research projects.

ASPRAY: Were there any examples that you can think of during your stay at IPTO where you went upstairs for approval to Herzfeld, or Lukasik, or whoever it happened to be at the time? And you were given the message that this may be too far in a basic research mode, not obvious enough connection to defense needs?

TAYLOR: No.

ASPRAY: There was nothing of that.

TAYLOR: No.

ASPRAY: Can you make a few comments about...?

TAYLOR: By the way, let me point you to something. When Clark Clifford was Secretary of Defense he gave a major speech that in effect proposed that the Defense Department take on the role of supporting research across national interests, not just defense interests. His argument was the Defense Department was best set up to

evaluate, procure, and administrate research, because they had the largest such organization in the world. They had all the administrative and logistics talent necessary to do that. It's ironic that after his time there the Defense Department turned in the opposite direction, and began to constrain its research rationale more and more to the military mission. I'd recommend you try to find that speech and read it; it's really remarkable. I'm sure you can find it.

TAPE 1/SIDE 2

ASPRAY: Can you make a few comments about the working relationship that you had with the DARPA director, or the DARPA deputy director? What kind of freedom did you have? What kinds of flexibility was there in your budget? What kind of reporting relationship did you have?

TAYLOR: Enormous freedom. While I was there, the DARPA directors, for the most part, were very concerned with getting on with the job. They were not at all enamored by bureaucratic mechanisms or policies. They, by and large, cared about the nature of the technical problems, or the scientific problems, or the challenges thereof. I can't remember any instances where I was given a directive by any of these fellows -- thou shalt, or thou shalt not do this, that, or the other. They would occasionally ask for help on various matters. Probably most significant instance of that came as the Vietnam war was building up. You may recall the war was essentially being run by Johnson out of the White House at this time, and there were discrepancies in reporting that was coming back from Vietnam to the White House about enemy killed, supplies captured, bullets on hand, logistics reports of various kinds. The Army had one reporting system; the Navy had another; the Marine Corp had another. It was clear that not all of these reports could be true. I think one specific example was that if the amount of sugar reported captured were true we would have cornered two-thirds of the world's sugar supply, or something like that. It was ridiculous. So, out of frustration, the White House turned to the Secretary of Defense to clean this mess up. And the Secretary of Defense turned to ARPA, because ARPA was a quick-response kind

of an agency then. The director of ARPA asked me to go out to Vietnam and see what the situation was, and see if I could do anything about it.

ASPRAY: Why would he ask you, as opposed to one of the other technical..?

TAYLOR: Because it was fundamentally a logistics and reporting problem, which suggested that if it was going to be better coordinated, better controlled, that computer technologies would likely have some role. That was one of the hypotheses. One of the reasons I was to go out there was to see whether or not any kind of computer technology could bring at least some semblance of agreement, if not sanity, to this whole process. So I went out there on a number of occasions. On the first occasion, I talked to various people who were submitting these reports back to Washington. I got a sense of how the data was collected, how it was analyzed, and what was done with it before it was sent back to the White House, and I realized that there was no uniform data collection or reporting structure. In another contract that was my responsibility at SDC, I had come to know a smart chap named Dick Beeler who I thought could bring some order into this chaos. So I asked SDC to free him up to go out there with me on another trip. He surveyed the situation with me and agreed. So I funded him to put together a working group of people under his direction who would go out to all the services and redesign their data collection and reporting structures to make them consistent and uniform. Then I funded him to put a team together to get a computer installed at MACV, at the command headquarters at Tonsinook (?) Air Force Base -- Military Assistance Command Vietnam is what MACV stood for. I think Westmoreland was probably head of it then; I can't remember. So they built a computer center at Tonsinook and had all of this data come in through there. After that the White House got a single report rather than several. That pleased them; whether the data was any more correct or not, I don't know, but at least it was more consistent. That resulted in several trips to Vietnam. So there was the occasion where I was asked by ARPA to do something, but those were very seldom.

ASPRAY: In a period in which other offices within ARPA were increasingly coming to be heavily involved in the Vietnam conflict...

TAYLOR: That was the limit of our involvement. ARPA's overall budget during the Vietnam conflict, I believe, was cut in half. It went down substantially. But the IPT budget was doubled. None of the IPT supported research had anything to do with what was going on in Vietnam.

ASPRAY: Did this cause a certain amount of resentment within the office?

TAYLOR: Perhaps. Again, I didn't hear it; I would have been the last to have heard it probably. So I don't really know.

ASPRAY: Perhaps you can tell me something about the annual budgeting process. What sorts of procedures you went through?

TAYLOR: Well, we put together a budget each year, and we'd go to the director of ARPA's office. We'd sit down with him and go through it. It was very straightforward, relatively easy, sometimes almost pro forma. I can recall defending a 25 million dollar budget on the order of a 30 minute meeting. Sometimes the meetings were an hour, but they were never half a day.

ASPRAY: Did you have to defend your proposal against those of other offices?

TAYLOR: Not directly. No, not with other offices being present. We'd go through each of our projects, line item by line item, with the director of ARPA, and the deputy director, and his program manager (the fellow who ran the administrative part for ARPA, dealt with the contracting officers, etc.) and tell what each research contract was about, and what their accomplishments had recently been, what their problems had recently been. Over the

course of a year I'd try to get the director, and deputy director, and sometimes the program manager individually briefed on these projects by having people on the projects visit them in Washington. I'd take them in and they'd chat with the director of ARPA, the deputy director of ARPA, or both of them. Occasionally someone from the head ARPA office would visit one of our contractors. So I tried to make sure that the director of ARPA and his staff were not caught by surprise. One of the reasons that these budget meetings went as quickly as they did is because the director of ARPA and his staff were already familiar with most of these projects, and in some cases even familiar with the individuals who were working on them. That made the budget meetings run smoothly.

ASPRAY: When you prepared your budget to submit to these people, did you already have a dollar figure that had been given to you that you were trying to divide up, or were you building up from the bottom and trying to present a package?

TAYLOR: I was building up from the bottom. I may have been given a guideline figure from the director's office on occasion, but I remember not paying too much attention to it. I would build a program based on the quality of the proposal that we had in hand, the quality of the work, and the obligations that we already had committed ourselves to. These were multi-year programs we were funding. Most of them were supporting graduate students, and you can't jerk the funding around in a graduate student program, or else the poor graduate student is going to spend his life in graduate school; he's not going to be able to get his Ph.D. So, I think this is the way in which the budget was able to grow. It wasn't so much that the ARPA director was saying, "Well, we're going to give you more money next year," but rather we would come in with things that would represent additions to the program, and he would approve them. I don't know where, in most cases, he got the money to fund them.

ASPRAY: I see. So, in the typical year when you would present your proposed budget to the director and his associates, how successful would you be? Would more or less what you went in with be what you came out with?

TAYLOR: Yes. I don't remember proposing any huge changes. I mean, the IPT budget doubled while I was there, but it doubled incrementally; it didn't double overnight.

ASPRAY: Over a five year period, or so.

TAYLOR: Right.

ASPRAY: If I remember correctly, this is the period in which there was a great deal of criticism about multi-year contracts, especially in some of the other programs, for example in materials science. Was there some push to move you away from multi-year?

TAYLOR: Well, I wasn't multi-year funded in all cases. That is to say, if you looked at the work statement between my office and an individual contractor, there would not be a written budget plan that said, "We're going to fund you so much this year, so much next year, and so much in the third year." But once we made a commitment for one year to one of the graduate school, Ph.D. granting programs there was an unwritten understanding with the front office in ARPA that if we were ever going to reduce that commitment we had to do it very gradually so as not to upset the educational process. Now, I think the Carnegie program was funded three years in advance. And there may have been a few others, but most of them in a legalistic sense were only funded a year at time.

ASPRAY: I know that the Project MAC work was originally three years, and then somewhere... I don't have the data in hand, but it seems to me it right in the middle of your administration that it was cut back from three years to two years. And there's a long trail of records in the MIT archives about their being rather upset about this.

TAYLOR: Yes. That could well be. I wasn't particularly upset about it, because I believed that that funding was going to be forthcoming -- whether it was contracted up front to be forthcoming, or not -- because I understood what the philosophy of the ARPA directors was. If MIT was worried about it, they were worried about it unnecessarily. But of course they had no way of knowing that; a bird in the hand is worth two in the bush.

ASPRAY: Exactly. Let's move over to the program for a while. Could you categorize the funding areas during your period there? I mean, some of it was continuity from the previous directors. But what sorts of new things, and what sort of areas, if you were to identify four or five areas would you mention?

TAYLOR: Areas in the technical sense.

ASPRAY: Areas in the technical sense, that's right.

TAYLOR: Well, let's see. Some of the timesharing work was still going on while I was there. The Project MAC work, for example, was funded initially as a timesharing activity. As the systems aspects of the timesharing problem began to play out, then other aspects, such as other MIT researchers who were going to try to use and evaluate timesharing, would grow up. So on the one hand you might characterize the MAC work as timesharing work, but in fact over time its flavor changed from getting a new timesharing system up and running to trying to see what new applications timesharing might make more advantageous. Then you were into molecular modeling or some other domain in which the timesharing was simply a tool that was being used, as opposed to principal research thrust.

ASPRAY: And the IPTO office was funding this application work?

TAYLOR: Oh, yes. Yes. It didn't fund all the users of Project MAC by any means, but it funded a number of them. You'd have to call that computer applications research in some sense. But it got into other fields as well. I guess I would say that systems research was one principal domain in which we funded work, graphics was another, artificial intelligence was another. The systems work would include interactive computing of various sorts, but so would the graphics work. So the difficulty is that these labels are not crisp; they overlap.

ASPRAY: Can you give me a few examples of the graphic work that you were supporting?

TAYLOR: Yes. I started a graphic project from scratch at the University of Utah. The Michigan work was work I did not start, but was initially started as graphics research contract. Later...

ASPRAY: But it was started by a previous ARPA...?

TAYLOR: By either Ivan or Lick.

ASPRAY: I see, yes.

TAYLOR: Probably Ivan. I eventually canceled that contract, because I found that a lot of the resources, rather than going into graphics research were going into trying to build a timesharing system for their IBM 360/67, whose software did not work on delivery. I got annoyed that ARPA was helping pull IBM's chestnuts out of the fire, so I stopped the contract. That helped me start new contracts. If you stop a few contracts every now and then, well, the front office would look more kindly on you when you wanted to start new ones. So there were initiations there of both kinds -- of starting and stopping.

ASPRAY: Okay. You mentioned AI. What projects were underway then?

TAYLOR: I think most of the AI work of the country was funded by ARPA over that time period. Some of it had been started by Lick, some by Ivan.

ASPRAY: So Lick had gotten Minsky started.

TAYLOR: Minsky in those days was a part of Project MAC. And the Carnegie work. Carnegie-Mellon had what was really an umbrella computer science contract. The different pieces always had an AI component to it. Newell was the investigator. SRI had an AI project, which we funded.

ASPRAY: Who was the principal investigator?

TAYLOR: Well, there was Charlie Rosen, Nils Nilson, and a number of other people. I can't remember all of their names right now. They built a robot that got some notoriety, publicity -- an interesting project. Where else was AI work going on? There was some at Bolt, Beranek and Newman; not an AI contract per se, but just a piece of work under a larger scope, whereas the SRI work was an AI contract sort of on its own, and separate from the Englebart contract completely, which was not AI. I can't remember any other. I suppose there was some AI, what at least the people doing it would label AI, at SDC for awhile. But the SDC contract eventually went away. I can't remember if it went away during my tenure or later. But I do remember that the maintenance cost on that AFSQ32 computer was a million dollars a year, so that was the motivation to cut that work back after a while. That may have happened while I was there; I can't remember. SDC probably could tell you.

ASPRAY: Was there funding during your period of AI related programming language work, more LISP type work?

TAYLOR: Well, I think some AI work was always language-related, work on LISP, making LISP better in one way or another, building new varieties of it. I've always believed that all of that work is best described as

language research and not as AI research. It really doesn't have anything to do with AI whatsoever. It's a classical piece of language research where you're trying to figure out ways of expressing things to a machine that other languages for one reason or another won't allow you to express. It really doesn't have anything to do with AI. It's gotten swept under the AI rug, because LISP is a useful and favorite language for doing AI research. This is probably a good opportunity to say that in those days AI had really not separated itself from the mainstream of computer science. It saw itself as a part of computer science, and at Carnegie it was just one element in a program that had many elements. At MIT, the same was true. They didn't wear AI on their sleeves to the extent that they did later on. As such, because they were in a stronger connection with other researchers working on other computer science problems, they served as kind of an avant garde for the rest of the field. This was also true at Stanford. I forgot to mention the AI work at Stanford. That was an important component of the AI work that we sponsored over this period of time. John McCarthy... A number of the people who might be called AI people in those days were also good systems people. Jerry Feldman, for example, who later went to Rochester might be regarded as an AI researcher, but he was also a very good systems researcher. At any rate, the AI people would often say, "Well, I'd like to build a system that does the following." It's a hand-eye system, or it's a robot, or something of that sort. They would tackle system objectives that forced them to do systems research that was way out in front of other systems research of that time. So they provided a leading edge where they addressed some hard problems early on. A lot of the knowledge that they gained from that work fed through the rest of the computer research community. A synonym for being the leading edge might be the lunatic fringe. But they provided a service because they were challenging other people and some of their own colleagues to tackle hard problems and they were finding ways in which components of systems which previously had not been attempted in fact could now work together (for some level of "work"). They were making progress. But as AI began to build what it called an AI community they began to separate themselves from computer science. At MIT, for example, they put themselves in a separate organization. They got separate funding. And I think the whole field of computing research and the AI field in particular began to get weaker at that point and has never recovered from it. Over the years I've interviewed lots of new Ph.D.s, and it's possible to find people who went through a curriculum some place that was fundamentally or principally an AI curriculum. And when you ask

them about operating systems, or data structures, or compiler design, they're ignorant. Consequently, they're not very strong computer scientists, because they're missing some important elements of mainstream computing technology. If that split had not occurred I think that we would see a different result -- a better result.

ASPRAY: So that's an explanation of your comment that you made before we were on tape at the beginning of our discussion about two successes and one failure.

TAYLOR: Right.

ASPRAY: At about this same time, the National Science Foundation was putting quite a bit of money into natural language processing.

TAYLOR: The National Science Foundation was the other member of Licklider's committee that I failed to mention.

ASPRAY: Was there any ARPA money at the time in natural language processing?

TAYLOR: Yes, there was natural language work at Bolt, Beranek and Newman. There may have been some at Carnegie-Mellon; I can't remember. There might even have been some at SRI and Stanford. There probably was.

ASPRAY: It sounds as though from your comment that while there was some work going on, it wasn't in any real sense a very major part of the program of your funded research. I mean, it dominated NSF's funding in computing in this period.

TAYLOR: Did it really? Are you sure? I would have thought that theoretical computer science has always dominated NSF's budget.

ASPRAY: Well, through something like 1967 a vast majority of the money was going into natural language processing.

TAYLOR: Well, but they didn't have a very large budget either.

ASPRAY: That's right; they didn't.

TAYLOR: In those days they were funding people typically in $35,000 a year, $50,000 a year, or maybe $70,000 a year chunks -- and funding individuals. ARPA's style was to fund large projects, or at least larger projects -- team projects. I don't remember a single case where we ever funded a single individual's work. This, again, is back to this plan of trying to make an order of magnitude difference. I think we were trying to fill a niche, in some sense, that we thought other agencies would be unable to fill because of their limited budgets. The individual researcher who is just looking for support for his own individual work could [potentially] find many homes to support that work. So we tended not to fund those, because we felt that they were already pretty well covered. Instead, we funded larger groups -- teams.

TAPE 2/SIDE 1

ASPRAY: I'd like to pursue that issue a little bit further. The common wisdom about the differences between NSF and ARPA are that NSF is much more democratic; ARPA is much more elitist in its funding patterns in some ways. That is, that ARPA would make bets on a few top research centers and then continue to support those -- run with the horses, so to speak. Do you want to talk about that philosophy? How conscious was it in

your mind? Were there efforts on your part to build up new centers of... Well, in fact, Licklider talks about centers of excellence.

TAYLOR: Yes. I do want to comment on that. There are elements that ring false and ring true in this comparison. It's true that the center of excellence notion was one that we subscribed to at ARPA. Indeed, what I was trying to do with the project at the University of Utah was to build a center of excellence in graphics. So that was something that was important to ARPA. Now, with regard to whether or not that's elitism, or on the other hand whether or not having a committee decision structure as the NSF does is really more democratic, I think is subject to argument. I can just as easily argue that having a committee of folks make a decision about supporting a particular proposal that goes to the National Science Foundation easily is an example of elitism. That is to say, there is this club of people who all work, let's say, in natural languages. There are a set of people who are accepted in that club, and there are a set of people who aren't. You float that proposal around to a group of people that NSF has chosen to be its peer review group. And if you're in the club you might get accepted, and if you're not you might not get accepted. So, I think I reject the notion of elitism versus democracy, and just say there was a different set of objectives, a different modus operandi. ARPA initially was to be a quick reaction operation -- let's see where we make a big difference in a few places. ARPA had no commitment, for example, to take geography into consideration when it funded work. There were no considerations whatsoever that, well, we're funding too much work on both coasts; we had better fund some work in the middle of the country. That was never considered. It's not that it shouldn't be considered. NSF certainly considered it, and there's nothing wrong with considering it. But the two organizations had a totally different value system in the way in which they went about doing what they were doing. They also had a different constituency in some sense, or a different set of oversight processes. The Congress looked at the National Science Foundation quite differently than the way they looked at the research aspects of the Department of Defense. The research pieces of the Department of Defense as compared to the development pieces of the Department of Defense were minuscule, whereas the National Science Foundation was in toto a research organization. So there were, indeed, a lot of differences, but I think those differences have to be evaluated based on their own individual pluses and

minuses, and not on a comparative evaluation. There are reasons why the NSF program worked the way it did: there are good reasons. And there are good reasons why the ARPA program worked the way it did.

ASPRAY: Let me see if I can... How shall I put this? Well, maybe the best way is by an example. When I went to talk to Marvin Minsky about support for early research in what's now known as AI, he said that there was all the difference in the world; that when he and other researchers would go to the NSF in the early days of their funding in computing, there was an attitude that did not favor artificial intelligence research, and that no proposal, however meritorious, could successfully make it through the NSF peer review at that time, but that if there were imaginative, daring people in the offices at DARPA that they would be willing to take the risk and support something that perhaps a large portion of the community, or at least part of the community that had some influence, was not willing to support. He saw the DARPA style as being particularly influential in the AI area, because he didn't see any support coming from NSF because of their peer review process. Do you agree with that? Do you see other examples of it? Do you want to comment on that?

TAYLOR: I think that ARPA's support of AI during the period where AI was more strongly coupled into the rest of computing was a beneficial influence on the rest of computing. I think that later, as AI became separated from the rest of computing, and the ARPA investment in AI in fact grew, that it was a mistake on ARPA's part. I think that the AI growth over the 1970s in ARPA support, by and large, has not paid off. It has not benefitted the rest of computing, and it has not given us new technology. So I don't think that the distinction between what NSF would support and what ARPA would support is the crux of the matter here. The AI people, who were getting support from ARPA when I was there, may have thought that the reason that I was supporting AI was because I believed in AI, qua AI. If they thought that, they were mistaken. I was supporting it because of its influence on the rest of the field, not because I because I believed that they would indeed be able to make a ping-pong playing machine in the next three years, but because it was an important stimulus to the rest of the field. There was no reason for me to tell them that, of course.

ASPRAY: No.

TAYLOR: But as they were no longer able to stimulate the rest of the field --in fact, as they began to annoy and alienate the rest of the field, in my judgement, the reason for supporting them got considerably weakened. And today, a large part of the rest of the field is alienated considerably by a lot of the AI hype that goes on.

ASPRAY: Let me move away from AI and try to get back to the underlying question there though, which may or may not have any further need for discussion; that is, because NSF and DARPA are different kinds of organizations with different resources and different styles of promoting research...

TAYLOR: And I think different raison d'etres.

ASPRAY: Yes. It's quite conceivable that one or the other might be more effective at promoting research in one way or another.

TAYLOR: Yes.

ASPRAY: Can you elucidate some of those for me? For example, can you give me ideas of programs that you funded which you think may not have been very successfully or effectively funded if they were under the guise of NSF?

TAYLOR: Okay. I think this kind of question starts with the way in which Congress views, or has viewed, at least in the past, the National Science Foundation versus the way it views the Defense Department. The Congressman's views are a reflection of what the Congressman thinks will have the maximum effect on his ability to get reelected by his constituency. I think many Congressmen believe, perhaps correctly, that most of their constituency are not really very interested in their Congressman's position on the financial support to the National

Science Foundation. I think many Congressmen believe, perhaps correctly, that their constituency thinks that the National Science Foundation must be some kind of organization that supports a lot of eggheads, people in ivory towers who do funny studies that they hear about from William Proxmire that don't make a hill of beans to anyone; and so there's no reason why they should be particularly interested in it. Well, on the other hand, most Congressmen believe that their constituency wants that Congressman to care about national defense. And in some parts of the constituencies, if they think their Congressman is not supporting national defense, then they question his loyalty to the United States. So, as long as we have that kind of perceived value system in the country -- whether it's the actual value system or not; and I think in some cases it is -- I think the National Science Foundation is always going to have a difficult time sponsoring research across the board the way, as an American citizen, I would like to see it sponsored. The Defense Department is always going to have an easier time, just in terms of getting the money. Now, what sort of other ground rules they put on top of that -- that is, these two supporting organizations in terms of how they spend that money... For example, I mentioned earlier the case of the Defense Department putting mission-oriented constraints on how they spend their money. It turns out to be, I think, a poor strategy. But with regard to the question of whether some research ought to be best sponsored by the National Science Foundation, or whether some research ought to best be sponsored by ARPA, it's got to start first with the pragmatic question of who has the best chance of getting money at the scale needed to support the stability and continuity that all research support absolutely requires. If you don't have stability in research support, you've got nothing. And so that's got to come first. Consequently, I think, today we need both organizations substantially supporting long-term research.

ASPRAY: Well, to review the major programs that you supported during your period as director, simply from a matter of budget size, for example, you may have been in a position to provide support that wasn't possible for NSF.

TAYLOR: Yes, I think that I was.

ASPRAY: Which examples would you point to as illustration of that?

TAYLOR: All of our multi-million dollar per year contracts in ARPA. During the time I was there I don't believe that the NSF had a single multi-million dollar per year contract in computer research. It would have been very difficult for them to do so for two reasons: one was their budget limitations, and the other was their peer review process. The peer review process at NSF was not likely to provide an endorsement for a large, team-oriented research project.

ASPRAY: Yes. Was anyone else at this time giving that size of grant?

TAYLOR: Yes, early on the National Institutes of Health were and could. Later, they got mission-oriented about their computer research. In the early days, they could support computer research just in the interest of better computer technology, how it is going to help not only medicine, but everybody else. But, toward the end of the time I was in ARPA they began to have a new policy that said that they will not support computer research unless it's in specific, direct application to a particular medical problem or domain. I don't know what their state is today, but I wouldn't be surprised if it's pretty much the same.

ASPRAY: Everybody else I think of, even if they put lots of money in it, were usually buying hardware, like the Energy Department (for the National Labs), or were giving small grants, like ONR.

TAYLOR: The cause of the small grant phenomenon is not just the existence of government funding agencies who prefer to fund individuals; its cause also rests with the reward structure that sits inside universities. The reward structure in the university is to promote the individual. The individual publishes or perishes. Team publication is always weaker than individual publication in the university value system for determining tenure, and raises, and that sort of thing. Systems research, which is really where the big success stories in computing research have come from over the last 30 years, inherently requires teamwork, and lots of it. I think the two

largest changes in computing in the last 20 or 30 years have come from timesharing and networking, and then the merger of those into distributed personal computing. Yet systems research is most difficult to fund and manage in a university. All of those were examples of systems research. So it's a tough problem. I think that from the standpoint of providing quality education to a Ph.D. computer science student, a strong computer science department must have a strong systems research activity, or else the student is not getting the kind of education he ought to get. But the university value system works against that. The student, therefore, is not well served, and consequently, our society is not well served, which is why we have a huge shortage of trained systems people in computing today and a large number of computing-trained people who are ignorant in systems research. Something is really out of kilter. It's as though we haven't learned from our own successes.

ASPRAY: It's hard to break that pattern of individual attention into individual evaluation that's built into the academic system.

TAYLOR: Yes.

ASPRAY: Let me go back to something that we began this meandering conversation with. You were categorizing the various areas you supported: systems research, graphics, AI, and languages you mentioned. Were there any other areas?

TAYLOR: Give me a multiple choice answer. We didn't support any numerical analysis, per se. I'm trying to think of the other domains of computer science. Are there any that you have questions about?

ASPRAY: I hadn't anything particular in mind. I was just trying to get some sense of the overview of your program. Communications?

TAYLOR: Not as such. I mean, you could argue that the ARPANET was a communications project, but there's something to be said for the connection between the success of a project and why you think you're doing it. I think that if we had labeled the ARPANET project as a communications research project, or even today in computing research, if we label a particular project as a communications project and you think of it that way, after a while labels tend to influence how you think about things. There are some areas (I think communications is one) where if you take it out of a systems context within which you're doing research, then you're likely not to succeed, or you're likely to have a lower quality research program than you had. So, the objective in the ARPANET was to get these timesharing communities -- which had been built up locally as a community because they had a medium around which or through which to form a community, namely timesharing -- to build metacommunities out of these by connecting them. That meant you had to pay a lot of attention to what was in these nodes, and not just to what was going between the nodes. So we never thought of it as a communications project, but rather as a systems project.

ASPRAY: I assume that when you talk about systems that you also consider work on architecture systems work, so that parallel work...

TAYLOR: Right. I would call ILLIAC IV an architecture project rather than a systems project; but, yes, that would be an example of such. We also funded some theoretical work. John McCarthy's work, while he might have been labeled then (and now) as an AI person, also was (and is) a theoretical computer scientist. There was some theory work that went on at CMU; there was some that went on at MIT.

ASPRAY: Cornell?

TAYLOR: During my tenure at ARPA we didn't have any work at Cornell. But we didn't have a project anywhere that was only theory. Again, where we could we tried to provide umbrella funding with a number of

projects going on beneath that umbrella. Not all of our projects were that way, but many of them were. In those cases there would be theory work included as well.

ASPRAY: Perhaps this is from a later period, but I noticed as I read through ARPA orders that, oh, a place like BBN would quite frequently have work that was involved in transferring some of the basic knowledge that had been learned to the military, whether it was building an ARPANET node for some part of the Navy, or whatever. Was very much of your budget in this period 1965 to 1969 spent on trying to apply what you had learned to the military, to defense needs?

TAYLOR: There was effort more than dollars. I'll give you some examples. The military could not have access to timesharing until the commercial segment could provide it. That's where the military got its computing. So, we spent a lot of effort trying to get computer companies interested in timesharing. I guess you could say we spent a lot of money doing that, but mostly it came down to having spent the money at places like Berkeley. I convinced SDS (Scientific Data Systems) to consider a new timesharing system prototype, done at U.C. Berkeley and paid for by the taxpayer. Why don't they adopt it, do some engineering on it, launch it as a product? So we spent a lot of effort doing that kind of thing. I sponsored the organization of a large European computer conference in 1967 to introduce timesharing to Europe, and paid for and encouraged people at our ARPA-funded places that were doing timesharing to travel over there, participate in that congress, give papers, run sessions, and so on. That's another part of technology transfer. The National Physical Laboratory in England hosted them. We spent some effort studying the defense command-and-control system -- especially their autodin network to try to find out what its users found wrong with it with a view toward the question of how well the ARPANET might overcome some of these problems. What were its latencies? What were its functional weaknesses? But while I was there we never gave money to one contractor in order to go and do x for the military, with the exception of this Vietnam situation that I told you about. And that wasn't a research problem at all; that was simply a logistics problem that was in a poor state of organization, and we got it organized. It didn't take a computer scientist to do that; it just took someone who understood the organizational logistics systems on

computers. Other program offices of ARPA had offices various other places around the world. They had one in Bangkok. So, when I was over in Vietnam I was encouraged to go over and visit this ARPA office and see what went on there. There was no information processing work there, but there were other ARPA things going on. I discovered that the census department of Thailand was essentially run by IBM and IBM employees. Census policy was dictated by IBM employees, because IBM at the time was the only computer company that could support its machines in that part of the world. That's why we had to put an IBM computer into Vietnam, because no other company had the service, maintenance, and support capability to maintain a machine in that part of the world. So IBM had the only computer serving the census bureau of the Thai government. And since the Thais who were normally in charge of the census bureau were not computer knowledgeable, they had to build their policies around whatever their IBM computer managers told them. I couldn't believe this. So, when Dick Beeler, the fellow who had solved this problem for us in Vietnam, finished, I said, "Would you like to go work in Bangkok for a while?" And he said, "Well, what do you have in mind?" I said, "Well, I believe that you can take an IBM 1130 and build a pretty good timesharing system for it." And he said, "Yes, I think that's right." So, I said I'd like to fund him to go into Chulalonghorn University, which is where most of the relatives of the power structure in Thailand get their education. I want him to build a timesharing system for the 1130. We're going to put it at Chulalonghorn University. And he is going to set up a curriculum in their engineering school to teach Thai engineers about computer systems technology around this timesharing system as a laboratory. I told him why I wanted to do it. And he did it. He spent three or four years there. He was a wizard with foreign languages. He picked up Vietnamese very quickly; he picked up Thai very quickly. Within a couple of years we began to have Thai citizens who could take jobs in the Bureau of the Census. So that was a lot of fun.

TAPE 2/SIDE 2

ASPRAY: I'd like to turn to your and the office's relationship with the contractors -- in particular, to understand a bit better how proactive you and your fellow officers were in the development of contracts and new initiatives;

also, to just understand what kind of working relationship you had with the contractors. Do you want to just begin to talk about that?

TAYLOR: Well, when you ask that question, the first thing I think of is how important I thought it was to get the researchers interacting, and the value of that. So, every year I organized an ARPA contractor's meeting, and I'd try to have it at some interesting place. Licklider probably started this; I can't remember. One year we had it in New Orleans during Mardi Gras; another year we had it at Snowbird, Utah during the ski season; another year we had it at the University of Hawaii. I can't remember the other places; maybe we had two of them at Snowbird. But anyway, at these meetings I would ask, over a period of a few days, each principal investigator to get up and give an hour or two description of his project: the sort of description he'd like to hear from each of the other projects. I think those meetings were really important. Through those meetings these people, all of whom were pretty bright, got to know one another better. I got them to argue with one another, which was very healthy, I think, and helpful to me because I would get insights about strengths or weaknesses that otherwise might be hidden from me. Through these we could look for opportunities where one group might be able to help another in working on some technical problem, or making some contact with some resource for some kind of supplier. I visited all the contract sites once or twice a year, sometimes more often. When I did that, I tried to find out who some of their brightest young people were: bright graduate students, new young faculty. I'd go in and I'd ask the senior principal investigator, "Give me some examples of some work that has impressed you that's being done by some of your graduate students. Give me some examples of some work that's being done by your youngest faculty. Who are your youngest faculty? What are they doing?" They would point me to these people and I'd go around and spend some time with them. That had prompted me to start, fairly early on an ARPA graduate student's conference, which was run pretty much like the ARPA principal investigator's conference. There I would ask each contractor to select two of their most outstanding graduate students to send at ARPA expense to some site -- typically they were held at a place outside Champaign-Urbana. So each contractor would send two graduate students to this conference. I wouldn't go; I was over 30. I sent a young fellow who worked for me named Barry Wessler to run the conference. He was on my staff and he had seen how

I ran the principal investigator's conference, and so I said, "Okay, I want you to go and just run these conferences exactly the same way. These guys get up. They talk about their projects under the same ground rules." At both of these conferences a lot of open time was left. We'd eat breakfast together. We'd have sessions in the morning. We'd eat lunch together. Then we'd break. People would be on their own to do what they wanted to do in the afternoon. Then we'd have dinner together. Then we'd have some sessions in the evening and then go to bed. Both these sets of conferences, I think, were important to the health and quality of the program. If there were technical weak spots in a program, then they would almost always surface under these conditions, because you could have a bunch of people around there poking holes in what you were saying or what you were doing. People that really cared about their work. They weren't interested in politics, they weren't interested in impressing anybody. If they thought that something that I was saying was dead wrong, they'd just as soon tell me as not. They'd just as soon tell one another as not if they thought they were wrong. So they were a lot of fun, very healthy, and I think they had a lot to do with the way in which we related to our contractors.

ASPRAY: What about when you were thinking about your program for the next couple of years? I mean, a lot of it was already built in. You had large portions of money already dedicated, but there were new initiatives, and certainly there were new contracts to continue work in certain areas, sometimes by new contractors. How did you go about transforming this notion of a program and then translating it into specific tasks and ARPA orders? For example, how much of this was generated from your conversations either at the PI meetings or on the road at the contract sites? Did you put out requests for proposals very often, or did you already identify the contractors that you had in mind beforehand and approach them? How willing were you to listen to a new idea by somebody he or she came up with one and looked for funding?

TAYLOR: Okay, the fundamental principle of operation for the office was to be in receipt and response of unsolicited proposals. We got a lot of proposals, only a small fraction of which we could fund. Typically, we'd get a proposal in and I guess it was seldom the case that we liked everything in the proposal, but maybe even occasionally we got proposals like that. Then we would invite the proposer to come in and talk about the

proposal, if we liked it. Through those conversations we would then try to shape a work statement. All the proposals came with work statements. We might just adopt a work statement as it was written, or we might try to convince the proposer that he ought to modify this element or that element of the work statement, or this element or that element of the budget. Maybe we thought he had allocated too little dollars to his computing costs, or too many dollars to his space costs, or too few dollars to his salary costs. We almost never sent out RFPs. In the time I was there I think the only RFP that I remember, but certainly the most important one we sent out was for the IMP -- the Interface Message Processor for the ARPANET.

ASPRAY: Oh, yes. Who would build it, right. In fact, that was an RFQ, wasn't it?

RFQ vs. RFP?

TAYLOR: Probably yes, it was an RFQ. That brings up an anomalous case to a normal method of operation. I had no proposals for the ARPANET. I just decided that we were going to build a network that would connect these interactive communities into a larger community in such a way that a user of one community could connect to a distant community as though that user were on his local system. First I went to Herzfeld and said, this is what I want to do, and why. That was literally a 15 minute conversation. And he said, "You've got it." He said, "How much money do you need to get off the ground?" I think I said a million dollars or so, just to get it organized. There was no ARPA order written or anything for months, maybe even a year later. Larry Roberts actually wrote the ARPA order long after we'd already gotten going. After I got Herzfeld's okay to go ahead, I began to go talk to various ARPA contractors about this idea. Most of the people I talked to were not initially enamored with the idea. I think some of the people saw it initially as an opportunity for someone else to come in and use their cycles. They never had enough cycles. But Licklider was very supportive. And Wes Clark was supportive.

ASPRAY: Who was Wes Clark?

TAYLOR: Wes Clark is one of the pioneers of computing. Do you know about the LINC?

ASPRAY: Yes.

TAYLOR: The first personal computer -- designed by Wes Clark and his team. He was also the originator of the idea that the ARPANET should be managed by these IMPs (Interface Message Processors) at each individual site rather than a large central machine some place in the middle of the country, which is what some people had been thinking. Wes Clark also was one of the designers of the TX2 at Lincoln Labs. And Wes Clark had good influence over Ivan Sutherland, Larry Roberts... a lot of people you know, including me. Of course Ivan did his sketchpad system on the TX2. Well, in spite of this sort of lukewarm reception to this networking idea, I was determined to go ahead.

lukewarm response.

ASPRAY: This was when?

TAYLOR: I saw Herzfeld early in 1966 -- February, I think. But I needed a program manager to run this whole thing. There was going to be a lot of work to do. I knew that Larry Roberts was interested in networking. In fact, I had asked him about some networking reliability questions that troubled me some months previously. He had gotten Computer Corporation of America to do a cross-country experiment with System Development Corporation to send some bits back and forth over some phone lines to get some questions answered. So I said, "Larry, I want you to come down to ARPA and be a program manager for this networking project, and I've got Herzfeld's okay." And he said, "No, I don't want to leave Lincoln Lab. I don't want to become a program manager at ARPA. I want to stay close to technical work doing what I'm doing at Lincoln Lab." This was probably late February when I started working on it. So he turned me down, and I would think of other people to get instead, but nobody really satisfied me, so I'd go back to Larry. He turned me down again. This happened frequently. And then one day in about September or October of 1966 it dawned on me that ARPA supported 51% of Lincoln Labs. So I went to see Herzfeld and I said, "Do we still support 51% of Lincoln Lab?" He said, "Yes." I said, "Do you remember this ARPANET... this networking project (I don't think we called it the ARPANET yet) that I'm trying to get off the ground?" He said, "Yes, how is it coming?" I said, "Well, I want

this program manager out at Lincoln Labs. His name is Larry Roberts, and he keeps turning me down. He's really the right guy to run this thing." I said, "Would you call (I think it was) Jerry Dinneen (who was the director of Lincoln Lab at the time)... Would you call him and tell him that it's in Lincoln Lab's and ARPA's best interests for him to tell Larry Roberts to come down and do this?" He said, "Sure." He picked up the phone with me in his office, and he said essentially that, and within two weeks Larry accepted the job. I remember he came down to Washington from Boston in December. Because he didn't have a house, he and his family stayed with me over the Christmas holidays in December of 1966. So he really wasn't on board and up and running until early 1967. And then it took off. That bit of history wasn't in that BB&N report that you mentioned earlier as to how it really got underway. I blackmailed Larry Roberts into fame!

ASPRAY: I had heard part of that story but not all of it. I had a question about this early history. In some of the notes that my colleague, Arthur Norberg, had prepared for me for this interview, he said that during 1965 Ivan Sutherland had had some interest in a similar idea, and had made some effort to get UCLA to link its various computing facilities together. But that's all I know about this.

TAYLOR: Yes. I was involved in that. I never thought of those two as connected, but, yes... The reason I didn't think of them as connected is because I saw that as having somewhat different motives. There were different computing... What should I call them...? Different groups -- three different groups at UCLA that had computer knowledge, computer ability, computer people collected around them. One was in the engineering school, one was in the business school, and I can't remember where the third one was. Ivan saw this as an opportunity to get these guys working together. But I think, without realizing it, he stepped into a political hornet's nest, because each of these three groups had leaders who didn't want to work together for one reason or another. Ivan tried to penetrate this structure, and I was with him on these trips -- most of them. I think maybe there was one chap -- one of the deans at UCLA who really did want to pull this together, and he asked Ivan for help. Ivan thought, "Yes, this would be a good idea." He tried to help them, but they just couldn't get them to cooperate. But they weren't talking about an interactive network, because they were not all interactive

systems. They were just talking about a network where they could have a compatibility across these systems, and at least do some load sharing, and some program sharing, data sharing -- that sort of thing. Whereas, the thing that struck me about the timesharing experience was that before there was a timesharing system, let's say at MIT, then there were a lot of individual people who didn't know each other who were interested in computing in one way or another, and who were doing whatever they could, however they could. As soon as the timesharing system became usable, these people began to know one another, share a lot of information, and ask of one another, "How do I use this? Where do I find that?" It was really phenomenal to see this computer become a medium that stimulated the formation of a human community. There wasn't any evidence of that in the UCLA case, because first of all, there were not new timesharing systems coming up at UCLA. UCLA didn't have any timesharing activities -- at least not ARPA-funded. Some of their groups, I think, were interested in interactive computing. They were interested in getting into it. But we weren't funding them to do it. And so, here ARPA had a number of sites by this time, each of which had its own sense of community and was digitally isolated from the other one. I saw a phrase in the Licklider memo. The phrase was in a totally different context -- something that he referred to as an "intergalactic network." I asked him about this later... recently, in fact I said, "Did you have a networking of the ARPANET sort in mind when you used that phrase?" He said, "No, I was thinking about a single timesharing system that was intergalactic...

ASPRAY: Very large, like an octopus?

TAYLOR: Right. So I said, "Well, what's clearly the right thing to do is to connect these places up and see if we can't make this interactive notion work at one level up." My memory tells me that later, when I mentioned to Ivan what I was doing, he took note of it, but he didn't comment on it one way or the other. He didn't either say, "That's a terrible idea," nor did he say, "That's a particularly good idea." But that was after he [left ARPA]. You said the records show that he left ARPA in June of 1966, but he was already out of ARPA -- not officially -- prior to that, because I was running the office by January or February, at least -- maybe earlier. I think he was

teaching at Harvard. Or maybe he was doing some research out at NSA. He had a project -- an off-site project of some kind; I can't remember where it was.

ASPRAY: So he was already off in another.

TAYLOR: That's why it was me, not him, that went to Herzfeld in February of 1966.

ASPRAY: Okay. Well, let's take the story on. So, in December of 1966 you were joined by Larry Roberts, who really comes to work at the beginning of 1967. It seems to me that the next major episode that I know of, anyway, in this story is the PI meeting that was held at Michigan, which seemed to be a place where...

TAYLOR: That's when we told the whole community officially what we were going to do. That's where Wes Clark...

ASPRAY: Introduced the IMP idea?

TAYLOR: Right. We call those "imps" -- we never spell them out, by the way.

ASPRAY: Oh, you do? Okay. Does that meeting stick in your mind in some way as being particularly important in the history of ARPANET, or was it mainly just a diffusion mechanism to tell others about what was going on?

TAYLOR: The latter. Two things stick in my mind about the meeting. One is that Englebart volunteered to be a network information center and to develop it, and we had some discussion around that. The other is that on the way to the airport from that meeting in a car is when I knew that Larry was leaning towards, or at least thinking about a machine in the center of the country to run the net. That worried me, and I had already told

Licklider that it worried me, and he had sympathized. Then I think I had told Wes Clark, because I knew Wes had a lot of influence over Larry technically. I think I told Wes prior to us getting in his car. But I might have introduced it in the car, I can't remember. Wes, and Larry, and I, and somebody else were in this car going to the airport to go home from a Michigan meeting, and I introduced the subject, and Wes said to Larry, "Why don't you just have a small (Wes believed in small computers)... Why don't you have a small computer at each site to do all of this?" He laid out a scheme, and Larry eventually bought it.

ASPRAY: The other piece of information that I have in my notes about this meeting was that Frank Westervelt was chosen to draft a position paper on the subject. Do you recall...?

TAYLOR: I remember Frank Westervelt, but I don't remember that. Did he do it?

ASPRAY: Well, let's see, my notes say that in mid-May of 1967 a draft was distributed and reviewed at a meeting. I assume that this was another PI meeting. Were they held twice a year by that time?

TAYLOR: No.

ASPRAY: Then I'm not sure what this meeting was. These are notes from my colleagues.

TAYLOR: It was probably a meeting called by Larry to help bring the thing together, is my guess.

ASPRAY: Okay. In the summer of 1967, a second draft by Westervelt and Mills was circulated. Was this Dick Mills?

TAYLOR: Probably.

ASPRAY: Yes. And then in October, 1967 there was a meeting at ARPA to discuss protocol and specifications for the IMP. And there was a request made to various individuals to estimate what the local traffic would be on the system by mid-1969.

TAYLOR: Well, Larry had contracted with an outfit in Long Island -- Network Analysis Corporation, I think it's called -- to do some of this modeling simulation.

ASPRAY: I see. Okay.

TAYLOR: Now, this position paper was addressed to whom for what purpose? You don't know?

ASPRAY: I don't know the answer.

TAYLOR: You probably should ask Larry.

ASPRAY: Okay.

TAYLOR: You can probably ask Westervelt to do that.

ASPRAY: Well, why don't you go ahead and tell me what happens then with the story of ARPANET from then on.

TAYLOR: Well, Larry took the ball and ran with it and got a specification written for the RFQ and sent it out. BB&N ultimately won it. Somewhere in there the director of the ARPA Behavioral Sciences office (ARPA had another program called Behavioral Sciences) left, and the director of ARPA asked me to take it over for awhile. Licklider had once been head of that office as well. So, I did that for a while. I can't remember for how long.

For some period of time I was running both offices. Therefore, some of those networking meetings and activities that I missed, because I couldn't be in two places at once...

TAPE 3/SIDE 1

TAYLOR: So I don't remember many more of the details, except that as each node would come up, it would be an exciting event. I guess I was also involved in that Vietnam thing during this same period, from time to time. And Larry really ran the networking project. I would get involved occasionally.

ASPRAY: One name that seems to come up a few times in here is Leonard Kleinrock's.

TAYLOR: Leonard Kleinrock was a good friend of Larry, and was very interested in being one of the network nodes in order to measure the net and its behavior. That's why you will see his name a lot. Larry depended on a number of people to help him -- a lot on Frank Hart at BB&N, a lot on the fellow at this Network Analysis Corporation who did some topological simulations -- Frank, I think maybe was his last name... Howie Frank... Howard Frank? Something like that.

ASPRAY: That name doesn't mean anything to me.

TAYLOR: Kleinrock. Who else?

ASPRAY: John Stehura.

TAYLOR: Where was he?

ASPRAY: All I know is that he was involved in working on control languages, but I don't I don't know where he was...

TAYLOR: BB&N maybe. I don't remember that name.

ASPRAY: I guess those are the only names I have. I don't want to continue this too long, but I have two or three other questions. I'm interested in seeing what another new initiative is like -- namely, the graphics story in Utah. Can you tell me a bit about that history?

TAYLOR: Well, Licklider had started a project at UC Berkeley with Harry Huskey and David Evans as principal investigators, initially simply to help Licklider monitor the progress at System Development Corporation to try to see what they could do with timesharing on this large computer, this AFSQ32 machine -- one of two built for the Air Force. So initially, Berkeley had this contract to have a model 33 teletypewriter and a leased line from Berkeley to Santa Monica to log into this thing from time to time, to evaluate what they found, and tell Licklider how they're doing. But Evans and Huskey had some bright graduate students who said, "Hey, let's build a timesharing system of our own." (And one of these graduate students I've worked with ever since, who I think is the most productive, has the most outstanding track record, and is the finest computer scientist in the systems area in the world. His name is Butler Lampson, whom you've heard of. He'll most surely win a Turing Award soon. He's very good. He was one of the principal designers of one of the early timesharing systems. His name is also on the Ethernet patent; he's one of the inventors of laser printing; he's done a whole host of things). So, that project at Berkeley grew into an early timesharing system project. They bought an SDS 930, tore it apart, put in memory protection, built a new operating system for it, and had a new timesharing system. There's a whole other long story about our efforts to get that adopted by SDS. Have you read this book, *Fumbling the Future*?

ASPRAY: Oh, I've seen it exists. I haven't read it, no.

TAYLOR: Oh, well, this story is in here. Have you read *Tools for Thought*?

ASPRAY: No, I don't even know that, I don't believe.

TAYLOR: Oh, well, you probably ought to read both of these. You might want to. Later, you can write down their significant information. So anyway, that Berkeley project, Project Genie, it was called, was a very successful project. There came a time when Dave Evans told me that he would like to return to his home, which was in Salt Lake City. He had been offered a position in the Computer Science Department at the University of Utah, which was a new department that they were asking him to chair and build up. He'd always been interested in graphics. So I said, "Great." Well, we talked about a work statement and I said, "Let's build a center of excellence in graphics at the University of Utah, and we'll fund it."

ASPRAY: What kind of work in graphics had ARPA supported up till this time?

TAYLOR: Well, they had supported some work at Rand Corporation -- Project Grail, it was called. There was graphics work that went on at MIT under a part of that umbrella Project MAC effort. There was graphics work at System Development Corporation. Where else? Those are the ones that I remember best. But there was no project devoted to graphics. The Michigan project was supposed to have been devoted to graphics but that one went awry; I told you about that earlier. That was it, I guess. So, I guess the Utah project was sort of our second attempt to build a research operation that was really graphically focused as its theme. So David went to Utah and got that under way. Ivan, meanwhile, had gone to Harvard. David recruited him from Harvard to come help him at Utah. They decided to form a company called "Evans and Sutherland," but they both stayed at the university for a while. They drew some good graduate students. I went there for a year when I left ARPA to sort of decompress from Vietnam. The experience of Vietnam helped convince me that it was time for me to leave ARPA. In fact, we had the ARPANET up and running, so I felt like I had been there long enough. You know, I don't think it's good for someone to stay there very long, really. There's sort of a czar mentality:

absolute power corrupts absolutely. So I went to Utah for a little less than a year. Anyway, some good graduate students were trained there. Two of them now run a DEC research center in Paris. Gourand Shading came out of there. Well, anyway, that's a whole other story.

ASPRAY: Sure.

TAYLOR: So that was sort of how that got going.

ASPRAY: The second of my last set of questions you partially answered, which was your leaving. Do you want to talk about the choice of your successor, and what was looked for?

TAYLOR: Well, he's the obvious choice. I recommended him, he still had a lot of work to do on the ARPANET, and he was willing to do the job so I don't think any of us gave much thought as to any other candidate.

ASPRAY: I see. Okay. The way I wanted to close was to give you essentially an open forum on tape to talk about any things related to ARPA that you wanted to talk about that we had missed, whether it's the larger picture, or later period, or whatever.

TAYLOR: Well, okay. There are two things that come to mind. I guess they both have to do with my opinions about how to make ARPA more effective. One is kind of at a global level, and the other is... There are two others at a local level. Let me speak locally first. Let's talk about it in terms of an existing ARPA contractor. An existing contractor comes to the end of his funding. He needs new funding to keep paying people. There might be slight changes in the work statement from one year to another, and so on. The stories I've heard are just mind-boggling as to the difficulty and length of time, and withholding [experienced with] the contractor not being able to pay people, having to borrow money from banks, because his ARPA money hasn't come in yet and

he's got to pay his people. This horrible lag time that sits somewhere between ARPA and the research group, probably in the contracting officer's domain, somewhere or another, has got to be fixed. There is no excuse for it. ARPA is suffering greatly. There are good people who just don't want to consider getting any kind of funding of that sort because of the irresponsible way in which that process has been dealt with.

ASPRAY: What's the history of this? When did this start to happen?

TAYLOR: Well, I suppose you could argue it started to happen back in my day... since before my day. ARPA once had its own contracting officer and contracting office. But early on they decided to use the services contracting officer or their contracting office. That happened either while I was there, or just before I got there. Now, those service contracting officers were pretty responsive to us. One of the reasons was that I hired a fellow named Al Blue out of the ARPA director's Program Management Office to be our guy in information processing who looked after these kinds of problems and stayed on the backs of these contracting officers to make sure they did the right thing. He was a guy who had been in the ARPA head office Program Management domain, so he knew that stuff cold. I convinced him and the director of ARPA to let him come over to work just for our office alone on the grounds that it would give him more opportunity for professional growth and advancement, which was true. He was wonderful. So he kept this kind of process running relatively smoothly for us, even though ARPA was no longer its own contract officer. He's now retired. He lives in Pennsylvania. I'm sure you can track him down. He'll know a lot of this history. I think I hired him after Larry came, but I can't remember. Anyway, the other local aspect of how to make the ARPA IPT activity improved has to do with this topic that we talked about earlier connected with an emphasis on paying attention to the young people who are in these research communities. I think that as ARPA IPT got bigger, there was more and more of a tendency to focus only on the principal investigators and only on the logistics, business, contractual, funding arrangements and less on the technical program, especially the sources of the ideas of the technical program. Those typically don't come from principal investigators. They typically come from the graduate students and professors who are doing research day in and day out rather than administrating research. I think ARPA IPT over the years has lost touch

with that. They don't run the graduate student congresses any more. I still visit lots of places that get ARPA funding, and that's part of the computer science community that is still my job to stay in touch with. When I've asked around various places, "Well, does anyone from ARPA come around and visit so and so, or this young person, or that young faculty member?" "No, not that I know of." Some people look at me like, "Why would he do that?" They've got to remember where the source of their ideas are and pay attention to it. So those are two things that I think [would make] ARPA [more effective] locally in the sense that those are small things, but they would make a large difference. These are not global issues, these two examples. The one global issue that I think is of utmost importance is this notion of mission-directed funding. Mission-constrained funding. I think that's enormously weakened the quality of the work that ARPA supports. The reason is that if you're looking for a solution to a technical problem where you're driven by technical limitations, you're trying to overcome these technical limitations. You've got a computer. It will do a lot of nice things for you, but it won't do this, this, this, and this. Or I can't make this system do this or that or the other. Or there is this theoretical problem that in-and-of-itself intrinsically is an important, interesting theoretical problem. Never mind what other external motivating forces might be. Never mind if the solution to this theoretical problem might help a particular defense operation do this or that or the other. Never mind that a particular set of system features would give us battlefield command capability. That's not what's important. What's important is you've got to focus on what the technological constraint or limitation is that you're trying to overcome. If you focus on that then you're going to do work that has enormous leverage. If you instead focus on an objective that says, let's see if we can't build a system to do this defense mission, or this part of defense mission, then you're going to miss opportunity after opportunity to make a real advance, because a large part of what we've done over the years is stumble on things. We didn't do it by planning, and if your motivation is really to see how we can understand this thing technologically and improve on it, then the Defense Department is going to gain and so will everybody else. I think it's a real blind spot. Also, I'm not absolutely convinced that it needs to be adhered to. I mean, there's a lot of interpretation going on here between what the Congress says, and what people in ARPA think the Congress says, and what the people who are actually carrying out the research think that ARPA thinks the Congress says. Bob Sproull, the older, and Charles Herzfeld, who followed him, and Bob Frash, were from time

Scientific statesmen.

to time asked by congressional staff people, or maybe even congressional committees, "How is this relevant? Tell me why this work that you're doing in ARPA is relevant to the Department of Defense." I know that there are instances where they would say, "That's not the right question to ask. The right question to ask is, 'How is this going to help the United States, or this technology, or our society, or our culture generally?'" They would stand up to these guys and in a polite, civilized way attack their myopia, because these ARPA directors were scientific statesmen. We have had too few of those people in that job since then.

ASPRAY: Okay. Thank you.

END OF INTERVIEW